Integrating the Choice of Product Configuration with Parameter Design

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Abstract

Parameter design and experimental techniques to develop new products essentially focus on the optimization of a single feasible configuration of a product. This paper, using an example, illustrates how to extend factor optimization to *system design* to support the designer in the choice of the product configuration.
The proposed approach integrates the design phases to reach the best configuration using a process aimed at subsequent improvement of the product, providing confidence in the information underlying the decision making process, and correctly testing all hypotheses and assumptions.
In this way, this paper is a further attempt to overcome barriers in industrial practice, by combining the engineering knowledge with effective statistical techniques.

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1 Introduction

The current concept of quality indicates that quality must be "designed in" the product, and that other forms of interventions present limitations and may not be cost effective [Unal R., 1993]. In typical product development practice "Quality by Design" consists of a three-step approach: *system design*, applies the available knowledge to produce a basic functional prototype; *parameter design*, identifies the setting of parameters which optimize the system performance; and *tolerance design*, establishes limits on allowable variation in the parameters [Dehnad K., 1988]. However, this methodology is only a partial upstream extension of design for quality, since it works only on a predefined product concept, and thus does not include the optimization of the product configuration.

Using the Powered Fruit Juicer example, in this work, we will show how experimental design can be used to evaluate complex architectural factors such as different design solutions for head shape, or various technologies such as two systems of head rotation, and how these choices affect final performance. Including *system design* in the experimental optimization, this approach moves the development process naturally toward greater integration.

The structure of this paper reflects the temporal differentiation in the underlying methodology between the *a priori* stage, in which the "product framework" and the "experiment pattern" are defined and the *a posteriori* stage in which, after experimentation, analysis and feedback is conducted to complete the development cycle.

1.1 Current Problems with System Design

The typical new product development approach in industry has been characterized by strong pressure to reduce time and improve quality, while the lack of planning and statistical tools are still strongly felt in all phases of design [Goh T.N., 1991]. One of the consequences of this development approach is a process where efforts are concentrated on the realization of the first prototype and its subsequent optimization through the definition of tolerances [Unal R., 1993].

Pushing to reach quickly a first feasible product architecture with subsequent row tuning of parameters under cost and quality constraints has already been demonstrated to be ineffective. As Taguchi pointed out a long time ago, to make products with high performance and robustness, a design process to optimize all key factors is necessary. This phase, known as *parameter design*, identifies the product's parameter settings that reduce the sensitivity to sources of variation. Even if this approach is essentially the classical Design of Experiment [Kacker R.N., 1991], Taguchi popularized this approach in industrial applications [Pignatiello J.J., 1992].

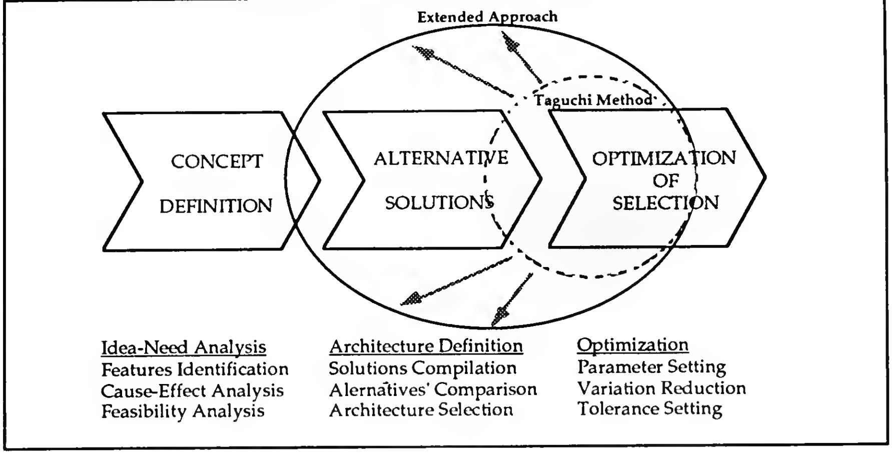
Concentrating efforts on the first selected product solution, in a strongly sequential way that does not allow either for subsequent iteration or ongoing feedback represent a weakness in this traditional approach. This tendency endures in spite of the possibility offered by the prototype as a milestone to close the loop in the early stages of product design, and of all verifications required to validate the assumptions of the statistical analysis.

1.2 Objectives of Proposed Approach

The present work points out the importance of and the possibility of extending factor optimization back to *system design* where the architecture of the product is not yet frozen (Exhibit 1). This main objective is also combined with the integration of design phases to:

- predispose and activate a continuous improvement process,
- test correctly all hypotheses and assumptions,
- know the reliability and confidence of the information underlying the decision-making process.

Exhibit 1. Extension of the approach in the development process.



These goals are intended to achieve in the design process flexibility and control. The flexibility is relevant because it avoids a situation in which, after the prototyping of product, the configuration is treated as fixed. In fact, the early freezing of the architecture has a strong impact either in finding the best overall solution for the product or in the creation of knowledge about the other alternatives. This, integrated with the feedback, becomes one of the keys to move the process toward the optimum, to formalize the knowledge and to avoid common mistakes occurring in the early stage of the design phases.

2 Approach

2.1 The "Powered Fruit Juicer" Example

An application of the proposed approach has been conducted on a consumer product, the Powered Fruit Juicer (PFJ), using brands already on the market as prototypes for the development of a potential next generation. This example will be used to illustrate the different steps of the present work, referring, in cases where the example does not apply, to hypothetical situations.

The Powered Fruit Juicers are home products mostly used for oranges, and all manufacturers offer products with similar designs. The juicers essentially work through head rotation while the user holds the fruit and applies pressure. These products exhibit some interesting features that are perhaps

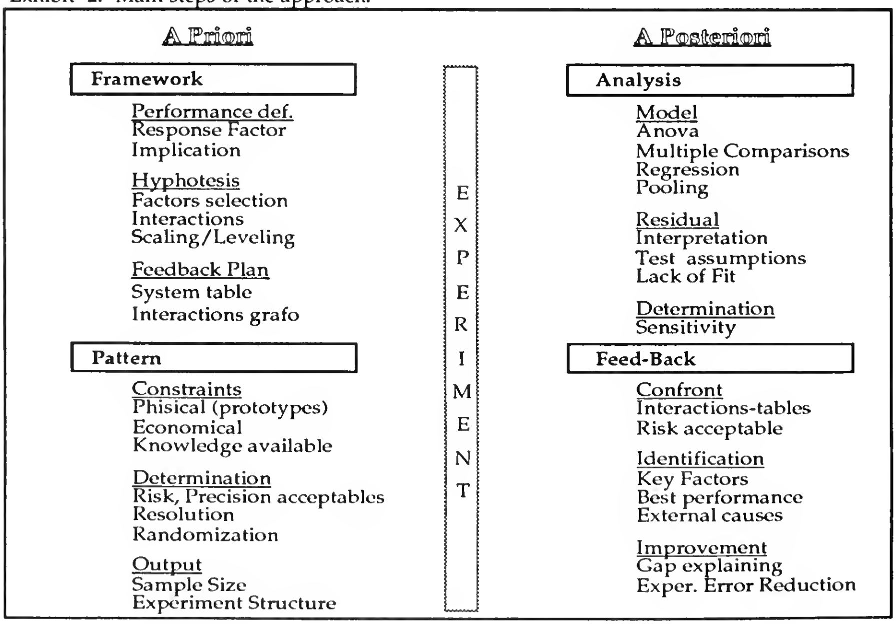
driven by the developer's experience and by marketing considerations rather than by objective performance evaluation. For this reason this example appears to be appropriate for an application of the proposed approach, which introduces architectural performance evaluation as part of the product design process.

2.2 Logic and Stages of the Approach

In our approach, two main concepts differentiate partially the design process from the traditional methodology. These are: the "framework of the product" and the "pattern of the experiment" (Exhibit 2). The first relates to the product/process under evaluation and synthesizes present knowledge about it. The second refers to the planned experimental process, respecting constraints and objectives, to improve the level of knowledge. The approach also identifies two stages of the improvement cycle, *a priori* and *a posteriori* of the experiment.

With only one iteration, like in the PFJ case, the design process provides the first best configuration of the product and updates related know-how.

Exhibit 2. Main steps of the approach.



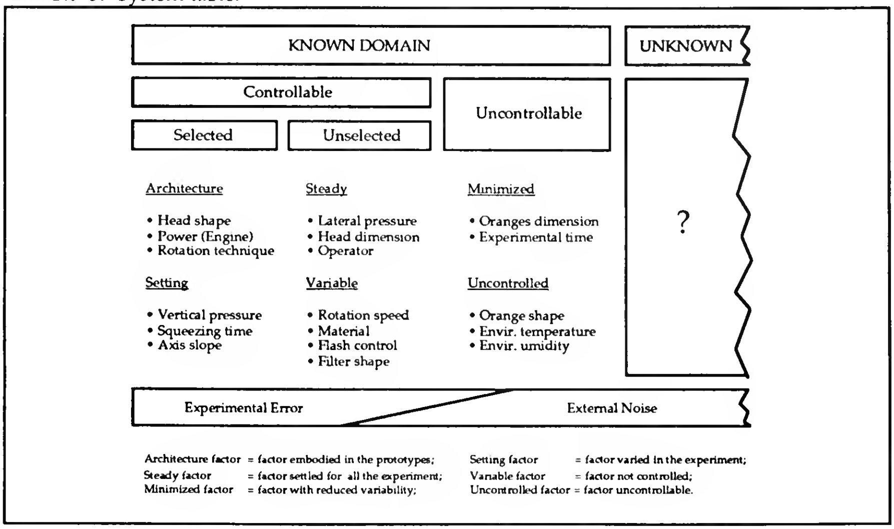
2.2.1 Framework of the Product

The concept of framework is an upstream extension of the product architecture. In fact, it includes the final architecture and other feasible alternatives, and is expressly defined to apply during the experimental phase. In this way the framework constitutes the experiment plan relative to the product and, although interrelated with the pattern of experimentation, it helps to simplify and to clarify the process.

In order to settle the product framework, as illustrated in the a priori stage of Exhibit 2 , it is necessary to determine the predominant performance of the product, the hypotheses to investigate and the feedback plan.

The performance under evaluation, and the response factor associated. This is a delicate point because, through a cause-effect analysis, the performance will be linked to the key factors, and the response factor can affect either controllable or uncontrollable factors. In the PFJ case the performance under evaluation was the juice yield, and the response factor was the ratio between the juice produced and the juice that remained in the fruit. The characteristic of this response factor is its very low sensitivity to the "juiciness" of a specific fruit, a critical element in the experimentation, but it remains somehow sensitive to the dimension and the shape of the fruit.

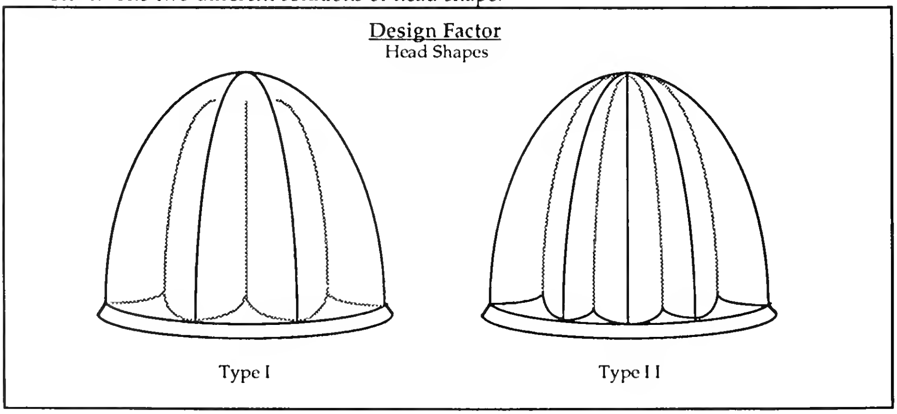
Exhibit 3. System table.



The principal hypotheses to investigate. These synthesize the engineering and scientific knowledge about the phenomenon under evaluation. They are:

- **the classification of the factors playing significant roles**, with the choice of *selected factors*, which are considered responsible for the main effects on the performance. Exhibit 3 shows the different categories using factors identified for the PFJ application with oranges.
  The selected factors, which will be directly observed, are divided into *architecture factors* and *setting factors;* the first group constitutes the peculiarities of the product concept or configuration. In the case of PFJ, this is composed of two design factors, (1) the head shape (Exhibit 4) and (2) the power, applied with different electric motors to the heads; then a technological factor, the head rotation scheme, where the first type is a one-way standard rotation and the second type is an alternated rotation in two directions (Exhibit 5). The architecture factors, as distinct from the setting factors, require more prototypes or a modular prototype with the possibility to exchange the parts during the experiment, and constitute the critical elements to investigate.

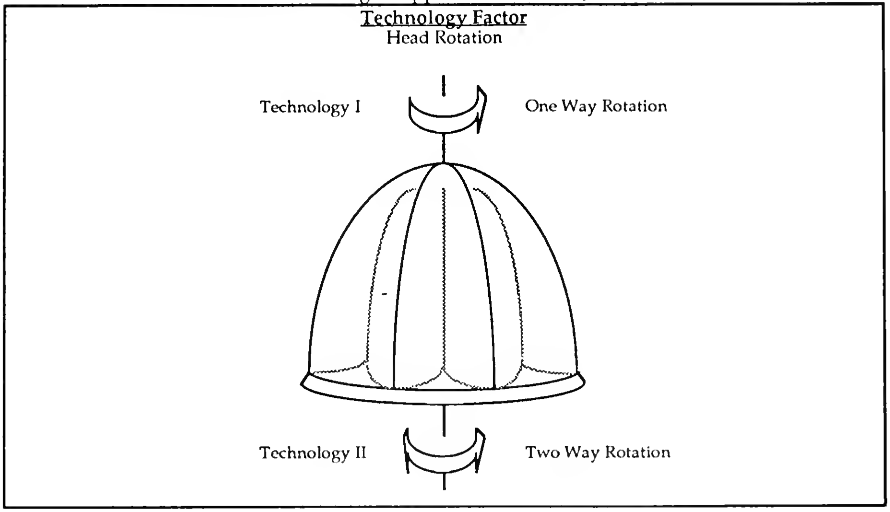
Exhibit 4. The two different solutions of head shape.



It is worth mentioning that not all categories identified for *unselected* and *uncontrollable* factors have to be present, but all of them influence the experimental results and are helpful for correctly planning the experiment. The choice of juice yield as response factor implies the minimization of variation in the orange dimension to reduce the uncontrolled variability in

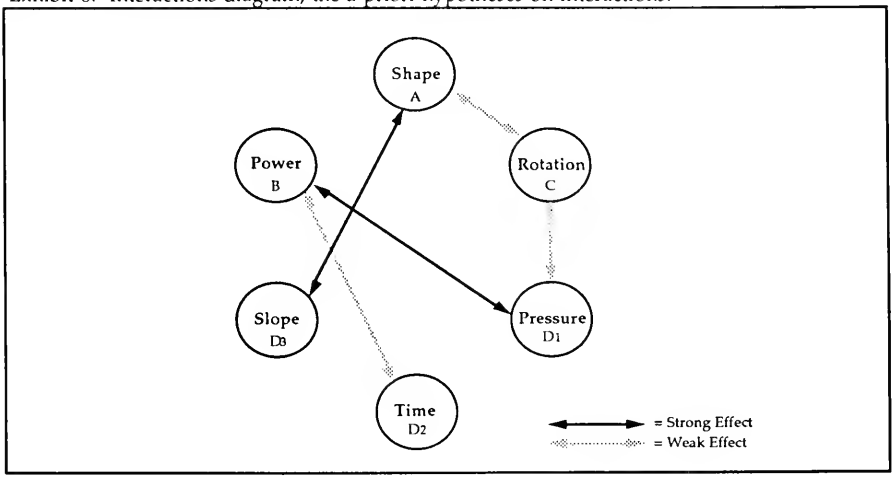
the experiment; while the shape of the orange was left uncontrolled because of technical difficulties in measurement.

Exhibit 5. The two different technologies applicable to the PFJ.



• **The interactions among the factors selected.** In table 6 is represented an interaction diagram showing the relationships among the factors and their intensities. These connections are generally based on previous experiences and reflect present knowledge.

Exhibit 6. Interactions diagram, the a priori hypotheses on interactions.



• **Scaling and leveling systems.** Assuming a metric and defining the levels at which the factors have to be observed, we can face factors that are not measurable and/or not continuous. In these cases, very frequent with architecture factors, it is necessary to divide the factors into different types (Table 1). For continuous factors, on the other hand, we must determine their levels. In the PFJ case the "range" was defined as the ratio between the difference of two levels and their middle point; this range should be significantly close to the percentage of the known or supposed variation of the factors. Erroneous definitions of levels can lead to altered conclusions, as we will see for the time factor.

Table 1. Leveling for the factors of PFJ case.

Letter	Factor	Level 1	Level 2	Range
A	head shape	Type I	Type II	-
B	power	18 watt	33 watt	67%
C	rotation	One way	Two ways	-
D1	pressure	0.12 Kg/cm2	0.08 Kg/cm2	40%
D2	time	10 sec	16 sec	46%
D3	slope	0°	20°	-

Feedback planning. Since one relevant part of the system design is the application of scientific and engineering knowledge to develop feasible configurations of the product, which is based on human intuition and experience, it is helpful to impose some structure to this process [de Falco M., 1993]. In fact, the improving-through-learning process [Deming W.E., 1986] requires that all information is synthesized and recorded in a form that allows subsequent comparison with new information, and makes this data accessible to those who can intervene in the design in its early stages [Box G., 1988]. Examples of possible formalizations are the system table and the interactions diagram developed in the PFJ example. Analogous work can be done with the well known cause-effect diagram.

2.2.2 Pattern of Experiment

The experimental phase is the stage where we try to understand better a phenomenon of the real world through experimentation [Winer B.J., 1970]. Since there always is a tradeoff between cost and the usefulness of data, it is

important to plan the experiment in order to achieve the right data at the lowest cost. This is particularly true in the prototyping phase, where we are often restricted by several technical and economical constraints.

The central point in this tradeoff is the number of trials to run during the experiment, which is linked with the number of factors and interactions we can investigate, and the precision of the data collected. Once this boundary is defined, there still are many parameters and structures to define in order to conduct the experiment that will yield the desired outcome. All these parameters along with the structure choices determine the "pattern of experiment".
The outcome of the experiment, although unknown, is completely defined once we have the framework of the product under study and the definition of the pattern of experiment in terms of design constraints and leverages.

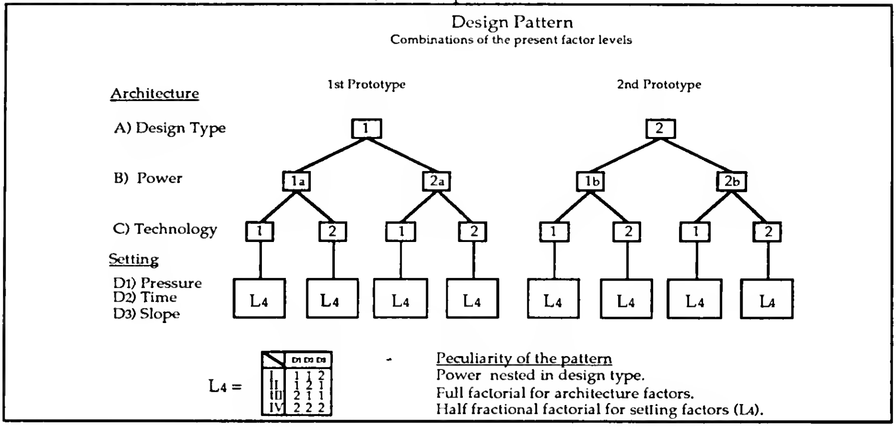
Design Constraints

- **Physical constraints.** Once all factors are characterized and the factors to control during the experiment are selected, it is essential to understand if some restrictions exist in the measurement of these due to the characteristics of the prototypes.

  Some of the typical restrictions that can occur are the "nested factors" and/or "split-plot factors". These are particularly frequent in prototype testing because architecture factors embodied in the prototypes are often not separable. In fact, each prototype can constitute a specific solution that, because of the different design, assembly and parts, can interact with other factors generating altered effects. In the PFJ pattern (Exhibit 7) we face a structure of prototypes where the power factor is nested in the design factor. Precisely because of the two different electric motors, the output levels of power are similar but not equal, even with the same power input. An easy way to recognize if a factor (X) is nested in another (Y) is to verify if each level of X is combined with every levels of factor Y [Mason R.L., 1989; Winer B.J., 1970].

  The main consequence of this restriction is that the effect of the power will be confounded with the design-power interactions, and these data will require a different treatment (Appendix A).

Exhibit 7. Pattern of the Powered Fruit Juicer experiment.



- **Economical-technical constraints.** These constraints determine the number of prototype variants we can build and the number of trials feasible, and so constitute the upperbound for the total number of runs in the experiment.
- **Knowledge constraints.** Since this approach is structured as an iterative process, the quality of the pattern, and then of the data, is linked to previous knowledge, of which a fundamental element is the error standard deviation of the experiment (σ_e). The availability of this parameter allows a better calculation of the minimum number of trials that matches our expected reliability.

Design leverages are the values to assign to the free experiment parameters to fit the strategy and design constraints.

- **Acceptable Risk level.** In the statistical analysis performed to identify the key factors, we can make two different types of error: Type I occurs when a factor is considered significant and in reality is not; and Type II occurs when a factor is considered not significant and in reality it is. We prevent ourselves from making these errors by fixing the probabilities of their occurring at a low level, P(Type I error)=α and P(Type II error)=β. In the PFJ case we fixed α =0.05 and β =0.05, the unusually strict control of the parameter β is justified by the importance of avoiding the rejection of a new design solution if it makes an improvement in product performance.
- **Precision of the experiment.** It is the required capacity to detect a specific change in the average of a response factor due to a factor or interaction.

This value should be defined in accordance with the variation considered of interest. Moreover we should observe that risk level, precision and total number of trials are strongly interrelated and their choice must be made coherently [Diamond W.J., 1981]. In fact, a high imposed precision, without changing other parameters, implies a high risk of making type II error. In the PFJ experiment the precision was fixed as the capacity to detect a variation of $\delta=\pm4\%$ in the ratio chosen as the response factor.

- **Degree of freedom allocation.** Once the total number of trials (N) with previous parameters is defined, the total number of degree of freedom (dof) available in the experiment (N-1) is determined. However, there still is the possibility of deploying these dofs among factors, interactions and error. In fact, respecting constraints which require in our case one more dof for the nested factor and for each interaction with the nested factor, including more factors in the experiment means fewer dofs available for the error and more possibility to have confounded factors. This is a reason why a high factors/trials ratio is not indicated for high precision experiments.
- **Resolution of experiment.** The last choice to make is the definition of resolution, with related confounding, for each factor and interaction of interest in the experiment [Tsui K.L., 1988]. This choice, which completes the pattern of experiment, is important because it defines factors that will be deeply understood and factors that will remain somewhat uncertain due to confounded effects.

Table 2. Resolution and confounding for factors and interactions .

Factor	Description	Resolution	Confounding
A	Head shape	V	$A.D_1.D_2.D_3$
B	Power	III	$A.B + B.D_1.D_2.D_3$
C	Rotation tech.	V	$B.D_1.D_2.D_3$
D_1	Pressure	III	$D_2.D_3$
D_2	Time	III	$D_1.D_3$
D_3	Slope	III	$D_1.D_2$
B.C	Interaction	V	$B.C.D_1.D_2.D_3$
A.C	Interaction	V	$A.C.D_1.D_2.D_3$
$A.D_j$	Interactions	IV	$A.D_i.D_k$
$B.D_j$	Interactions	III	$A.B + B.D_i.D_k$
$C.D_j$	Interactions	IV	$C.D_i.D_k$

The resolution III is obtained when a factor effect is confounded with two-factors interactions, the resolution IV when a factor effect is confounded with three-factors interactions, and so on. [Robinson G.K., 1993]

Any *a priori* knowledge about the impact of some effects could be a helpful guide in the resolution decision [Alkhairy A., 1991].

Accordingly with the factor categorization made above, in the PFJ example, the architecture parameters were positioned in the highest resolution position, because an error in the architecture is more serious than an error in the setting of the parameters (Table 2). In this way we can perform few trials, reducing quality of the data only where eventual errors would be less serious.

3 The PFJ Experiment

The experiment is completely designed once we have determined the previous elements in the framework and in the pattern. The total number of trials can be calculated using tables available in literature [Davies D.L., 1956], which express the relationship $f(\alpha, \beta, \Phi, n)=0$ (Appendix F), where $\Phi= \delta/\sigma_e$ is the ratio between the precision of the experiment and the error standard deviation, **n** is the sample size of each group compared in the experiment (equal, for two level factorial design, to half of the total number of trials).

In the design of experiment of PFJ, because of a total lack of knowledge, we conducted a preliminary test to estimate σ_e [Davies O.L., 1978], which came out to be approximately 0.0277. In Table F1 of the Appendix F, with Φ=1.45 (=0.04/0.0277), we find n=14. The closest orthogonal array is n=16=N/2 and then the total number of trials will be N=32. Another system to determine Φ, if σ_e is not known, is to define δ as percentage of σ_e. This can be done with the help of some conservative considerations [Diamond W.J., 1981; Mason R.L., 1989]. The minimum number of trials so calculated (32) matches the reduction imposed by the resolution choices.

The resulting pattern of the experiment is a full factorial for the architecture factors and a half factorial for the setting factors, thus we obtained the resolution V for the architecture factors, a resolution III for the setting factors, and a resolution IV for the interactions between the factors of the two different categories.

The pattern is recognizable in Exhibit 8 thanks to the positioning of the architecture factor on the top of the matrix, where full factorial implies the presence of all combinations among the factors. On the side we have the

setting factors where the half fractional design implies the presence of only half the data, but the orthogonality of the design guaranties the possibility of comparing any factor at each of its two levels.

Exhibit 8. Matrix of experiment for the PFJ case with the results.

ARCHITECTURE

A	Type I				Type II						
B	1a		2a		1b		2b				
C	1	2	1	2	1	2	1	2	D1	D2	D3
O									1	1	
B	0.73	0.81	0.65	0.65	0.44	0.62	0.39	0.55	2		1
S	0.94	0.82	0.81	0.91	0.68	0.60	0.57	0.68	1	2	
E									2		
R	0.72	0.67	0.75	0.75	0.58	0.66	0.53	0.68	1	1	
V									2		2
A									1	2	
T.	0.55	0.71	0.57	0.62	0.47	0.40	0.40	0.43	2		

SETTING

Assuming third-order and higher interactions are negligible, the model initially tested is:

$$\eta_{ijklmn} = \mu + \alpha_i + \beta_{j(i)} + \gamma_k + \delta_{1l} + \delta_{2m} + \delta_{3n} + (\alpha\gamma)_{ik} + (\beta\gamma)_{j(i)k} +$$
$$+ (\alpha\delta_1)_{il} + (\alpha\delta_2)_{im} + (\alpha\delta_3)_{in} + (\beta\delta_1)_{j(i)l} + (\beta\delta_2)_{j(i)m} +$$
$$+ (\beta\delta_3)_{j(i)n} + (\gamma\delta_1)_{kl} + (\gamma\delta_2)_{km} + (\gamma\delta_3)_{kn} + \varepsilon_{(ij)klmn}$$

where, Greek letters represent the effects of the respective factors indicated with Roman letters, the products in parenthesis are the interactions, μ is the overall mean, j(i) indicates that the β effect is nested in the α effect, and the indexes i, j, k, l, m, n vary between 1 and 2 [Montgomery D.C., 1984].

Therefore, the null and alternative hypotheses to test in the analysis were:

H_O: $\tau_{k1} = \tau_{k2} = 0$; H1: at least one $\tau_{ki} \neq 0$;

for every k;

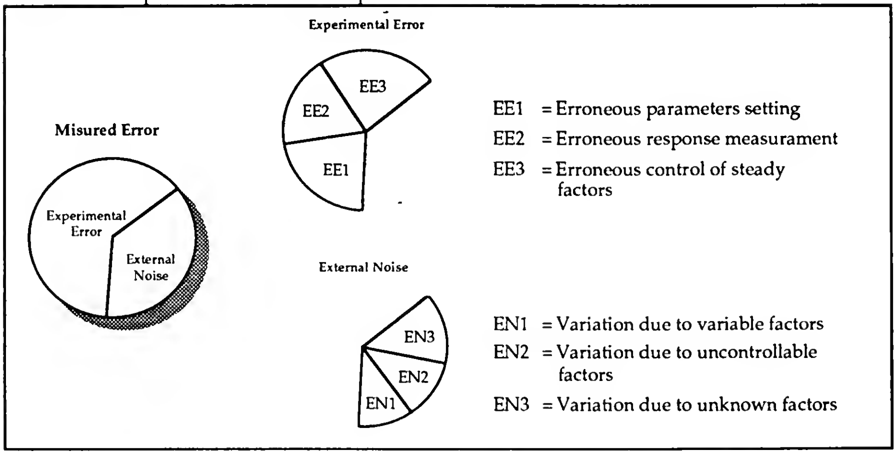
H_O: $(\tau_k \tau_h)_{ij} = 0$; H1: at least one $(\tau_k \tau_h)_{ij} \neq 0$;

for every k, h and for all i, j;

where τ_k represents the effect due to the generic factor under evaluation and $\tau_k \tau_h$ represents the generic evaluable second-order interaction.

Before starting the experiment, it is useful to check if all possible sources of error variation have been considered, because too high an error variation can compromise the ability to distinguish factor effects. The main error variations, according to the previous categorization of factors, are conceptually illustrated in Exhibit 9. From this description, we see that the preliminary error estimation could be underestimated, since this error did not appear the "parameters setting" component, because all the preliminary trials were made at the same factor level. This is evidence of how the correct error can be estimated only by an analogous experiment.

Exhibit 9. Components of measured experimental error.



The experiment has been conducted with fixed factors level and randomized trials technique, which means that all factors were kept at defined levels and the sequence of runs was random [Montgomery D.C., 1984]. The randomization of trials is an argued point, because, against common perception, the randomization is very expensive and the tendency to conduct grouped runs often compromises the quality of the experiment [Pignatiello J.J., 1992]. The main difficulties are due to much setup and tuning to change between trials. This may significantly enlarge experimental time.

The random sequence and outcome of the 32 trials conducted is illustrated in Table 3, where two levels of factors are indicated with indexes 1 and 2.

Table 3. Random sequence of the trials and results of the PFJ experiment.

Trial	Factors Combination A	B	C	D1	D2	D3	tab	J.Produced (ml)	J.Lost (ml)	Yield η (%)
1	2	2	2	2	2	2	32	51.0	67.5	0.430
2	2	2	1	1	1	2	23	56.5	49.5	0.533
3	2	2	2	1	1	2	24	61.5	28.5	0.683
4	1	1	1	2	1	1	1	85.0	31.0	0.733
5	1	1	1	1	2	1	9	118.5	7.9	0.938
6	2	1	1	1	1	2	21	81.0	58.5	0.581
7	1	2	1	2	2	2	27	62.0	46.5	0.571
8	1	2	1	2	1	1	3	89.0	47.0	0.654
9	2	1	2	1	1	2	22	68.5	35.5	0.659
10	2	1	1	2	2	2	29	55.0	62.5	0.468
11	1	1	1	1	1	2	17	97.5	38.0	0.720
12	2	2	1	2	2	2	31	47.5	72.5	0.396
13	2	2	1	2	1	1	7	49.5	76.0	0.394
14	2	2	2	2	1	1	8	70.0	57.0	0.551
15	1	2	2	1	1	2	20	115.8	38.5	0.750
16	2	1	2	1	2	1	14	76.5	50.0	0.605
17	1	2	2	2	2	2	28	86.5	53.5	0.618
18	1	2	2	2	1	1	4	85.7	46.2	0.650
19	1	1	2	2	1	1	2	90.0	21.2	0.809
20	2	1	2	2	1	1	6	66.5	41.0	0.619
21	1	2	1	1	1	2	19	80.5	27.0	0.749
22	1	1	2	1	1	2	18	91.7	46.0	0.666
23	1	2	1	1	2	1	11	86.0	20.7	0.806
24	2	1	2	2	2	2	30	48.5	73.5	0.398
25	2	2	1	1	2	1	15	67.5	50.5	0.572
26	1	1	1	2	2	2	25	73.5	59.5	0.553
27	1	1	2	2	2	2	26	93.5	39.0	0.706
28	1	1	2	1	2	1	10	110.4	25.0	0.815
29	2	2	2	1	2	1	16	67.7	32.0	0.679
30	1	2	2	1	2	1	12	110.8	10.8	0.911
31	2	1	1	1	2	1	13	77.0	35.5	0.684
32	2	1	1	2	1	1	5	51.0	64.0	0.443

tab = is the sequential order of the combination in the original matrix.

To avoid typical forms of biasing in the experiment, it is useful to follow some elementary rules: Make some preliminary trials just to get confidence with the equipment. This reduces the effect of learning curve. Avoid any kind of intermediate analysis during the experiment. This involuntarily influences the experimenter to modify subsequent runs in the direction of expectations. Do not repeat a trial because it yields an apparently strange result.

4 The Experiment Analysis

The experimental phase is generally expensive and for this reason we face in firms the tendency to reduce it as much as possible. However it is easy to encounter the same brief approach even in the analysis phase. The explanation often is that, even if the analysis is relatively inexpensive, it requires a deep understanding of both engineering and statistics topics

[Phadke M.S., 1989]. In spite of abundant available literature, this problem still presents many obstacles to adoption in industry.

Exhibit 10. Different analyses involved in the approach.

Selected Factors	Analysis
• Architecture • Setting	<u>Key Factors Identification</u> - Analysis of Variance - Percent Contribution - Error Pooling - Multiple Comparisons - Regression
Influenced Factors	
• Steady • Minimized	<u>No Inference</u> It's not possible to make inference on the unchanged factors. Any architecture factor left out is an unchanged factor.
Free Factors	
• Variable • Uncontrolled • Unknown	<u>Residual Analysis</u> Negative inference from the test of the assumptions.
Experimental Error	
• Set-up • Measurement • Low control	<u>Assumptions Test</u> - Randomness (Plotting, Run Test) - Normality (Shapiro-Wilk Test) - Independence (Durbin-Watson Test) - Homogeneity (Plotting)

The main analyses to conduct are listed in Exhibit 10 and, although difficult to separate sharply, they are organized in terms of the factors they address and the types of outcomes. Most of the efforts are focused on the identification of key factors and on the assumptions test, while remaining factors are only indirectly analyzed [Anderson V.L., 1974; Hunter J.S., 1985].

The first step is the identification of the key factors of the phenomenon. The approach is based on a preliminary Analysis of Variance (Anova), which allows testing of the hypotheses of the model. We assume, for brevity, that the reader has some familiarity with this technique, points to the references for details [Dunn O.J., 1987]. In some cases this analysis can be conducted with available software. In cases, like the PFJ application, due to the complex pattern derived by the nested factors and the mixed factorial structure, it is necessary to formulate in an ad hoc fashion the expressions (Appendix A). Table 4a shows the initial Anova table for the chosen model. Note that three-factors and higher interactions are considered negligible and are pooled together to form the residual error with 9 degrees of freedom.

Table 4a. Initial Anova table.

Source	dof	SS	MS	F	p-Value	Π
A	1	0.273	0.273	54.944	0.000	42.39 %
B(A)	2	0.006	0.003	0.626	0.557	-0.59 %
C	1	0.018	0.018	3.581	0.091	2.03 %
AC	1	0.004	0.004	0.768	0.403	-0.18 %
BC	2	0.008	0.004	0.810	0.475	-0.30 %
D1	1	0.174	0.174	35.009	0.000	26.72 %
D2	1	0.000	0.000	0.013	0.912	-0.78 %
D3	1	0.060	0.060	12.073	0.007	8.70 %
AD1	1	0.002	0.002	0.349	0.569	-0.51 %
AD2	1	0.005	0.005	1.100	0.322	0.08 %
AD3	1	0.011	0.011	2.144	0.177	0.90 %
BD1	2	0.010	0.005	0.991	0.408	-0.01 %
BD2	2	0.000	0.000	0.027	0.973	-1.53 %
BD3	2	0.007	0.003	0.692	0.524	-0.48 %
CD1	1	0.005	0.005	0.912	0.365	-0.07 %
CD2	1	0.005	0.005	1.040	0.335	0.03 %
CD3	1	0.000	0.000	0.035	0.856	-0.76 %
Residual	9	0.045	0.005			24.36 %
Total	31	0.631				100.00 %

Table 4b. Anova table with error pooled.

Source	dof	SS	MS	F	p-Value	Π
A	1	0.273	0.273	78.736	0.000	42.63 %
C	1	0.018	0.018	5.132	0.004	2.27 %
D1	1	0.174	0.174	50.169	0.000	26.96 %
D3	1	0.060	0.060	17.301	0.000	8.94 %
AD2	1	0.005	0.005	1.576	0.223	0.32 %
AD3	1	0.011	0.011	3.073	0.094	1.14 %
BD1	2	0.010	0.005	1.444	0.258	0.49 %
CD2	1	0.005	0.005	1.491	0.235	0.27 %
Residual	22	0.076	0.003			17.00 %
Total	31	0.631				100.00 %

Table 4c. Final Anova table with error pooled.

Source	dof	SS	MS	F	p-Value	Π
A	1	0.273	0.273	73.229	0.000	42.59 %
C	1	0.018	0.018	4.773	0.038	2.22 %
D1	1	0.174	0.174	46.661	0.000	26.92 %
D3	1	0.060	0.060	16.091	0.000	8.90 %
AD3	1	0.011	0.011	2.858	0.103	1.10 %
Residual	26	0.097	0.004			18.28 %
Total	31	0.631				100.00 %

SS = Sum of Squares, MS = Mean Square; F = MS/MS Residual; Π = Percent Contribution.

Also supplied in the table is the p-Value, known as "probability value", which gives the smallest significance level at which the null hypothesis can be rejected. The smaller the p-value, the higher the chances that a factor or interaction is significant. Since high significance of a factor or interaction does not mean necessarily a strong impact on the final performance of the product, we also provide the percent contribution of each effect, which gives the percentage of total variation due to a factor or interaction (Appendix B). This information is often important; in fact, as we note in Table 4 for the head-rotation factor, the significant level obtained ($p<0.05$) is related to a low impact on performance variation $\Pi_R=2\%$.

Pooling Technique

Although this is a controversial step [Davies O.L., 1978], from Table 4a we can improve the analysis by observing that the power and the time with many other interactions have a high p-Value ($p>0.5$), and so their variation can be considered as a component of unexplained (casual) variation and pooled in the residual. Thus the residual reaches 22 degrees of freedom, which means more accuracy in the error estimation. We can also test better the remaining factors and interaction. Table 4b shows that the only interaction with a low p-Value ($p<0.1$) is the design-slope interaction, so we keep pooling the other interactions to obtain the final model shown in Table 4c [Ross P.J., 1988]

Table 5. Anova table for the final model.

Source	dof		SS	MS	F	p-Value	Π
EXPLAINED	9		0.555	0.062	17.818	0.000	83.00 %
Factors		4	0.524	0.131	37.834	0.000	80.79 %
Interactions		5	0.031	0.006	1.806	0.186	2.21 %
RESIDUAL	22		0.076	0.003			17.00 %
TOTAL	31		0.631				100.00 %
MAIN EFFECTS	5		0.535	0.1069	28.722	0.000	81.72 %
RESIDUAL	26		0.097	0.0037			18.28 %
TOTAL	31		0.631				100.00 %

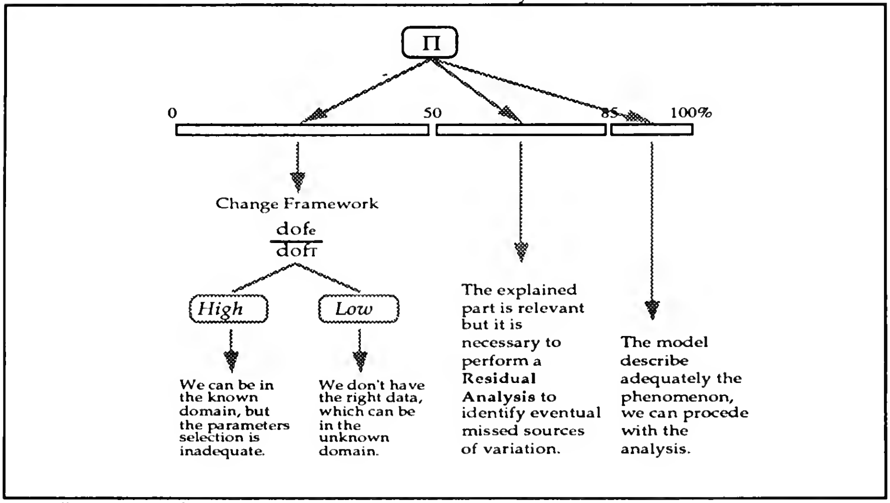
Contribution Analysis

The subsequent step is the Model Test and Percent Contribution analysis. In Table 5 we see the synthesis of the model analysis in terms of explained

factors, interactions and main effects.

From this information, beyond the significance of the model, we can determine the quality of the approach by observing the overall Percent Contribution (Π) of the model. Exhibit 11 shows the generic alternatives we can encounter. The bounds are arbitrary and only indicate a tendency rather then a rule. In the PFJ case, the 83% of contribution of the final model is sufficiently high, but still requires a careful analysis of the residual to explain eventual anomalies either in the framework or in the pattern of experiment.

Exhibit 11. Alternatives in the model contribution analysis.



Π = Percentage of the total variation explained by the model.

In the cases where the model contribution appears to be very low, we face the situation where it is likely necessary to change the framework. A row diagnosis of whether we have the right data or not can be made through the observation of the ratio between the dof available for the error and the total number of dof. When the ratio is high, we should check if everything in the analysis was conducted correctly and if one of the "not" selected factors is in fact relevant. The alternatives is that missed key factors are in the unknown domain or the key factors have been neutralized as steady factors. This is a pertinent point in any case, because during the definition of the framework, any architecture factor left out of the selected set behaves as a steady factor, and then it will not be possible to make any inference on it. In the PFJ

example, the head dimension was not included in the experiment, and so we did not know if it was a key factor. At the same time, we do not know if the performance has been significantly affected by the ratio of fruit and head diameter.

Residual Analysis

This part, often neglected, is essential to complete the preceding analyses and, most important, to verify if all assumptions underlying the methodology are met.

The analysis of residuals requires a previous regression analysis (Appendix C), from which we calculate fitted values and residuals. The assumptions verification and residual interpretation are two sides of the same coin. In fact, if we detect any pattern in the residual it means that some assumptions are not completely verified and therefore an unexpected effect can be in the residual, and vice versa [Montgomery D.C., 1984].

The assumptions of the analysis of variance are: normality, independence and homogeneity of residuals. There are many tests available in the literature and in some software to verify the normality and the independence of residuals, such as the Shapiro-Wilk test and the Durbin-Watson test. The last assumption is the most important, since the methodology is quite robust to the others [Dunn O.J., 1987]. A complementary and easy approach to accomplish this analysis is to plot the residuals against any of the factors and parameters that can be considered worrisome. In the PFJ residual analysis all assumptions were verified and in spite of the indication of model contribution it was impossible to identify any other form of unexpected variation. The obtained error variation was explained with the low precision of the experiment (Appendix D).

Sensitivity of Experiment

After the experimental phase, although defined in the pattern of experiment, it is useful to recalculate the precision of the experiment, now called "sensitivity", with the formula coming from the Fisher's Least-Significance Difference. This also gives the confidence interval for the difference of the means estimated [Mason R.L., 1989]:

$$\text{SEN} = \pm\, t_{[\alpha/2,\, \nu]}\ (2/n)^{1/2}\ S_e$$

this is relative to samples of same size, and $t_{[\alpha/2,\, \nu]}$ is the t-distribution with an upper-tail of probability $\alpha/2$; ν = degree of freedom of error; n = size of each sample compared; S_e = error standard deviation. In the PFJ example, where $\alpha = 0.05$, $\nu = 26$, $n = 16$ and $S_e = 0.0608$ we have:

$$\text{SEN} = \pm\, 2.056 * 0.354 * 0.0608 = \pm\, 4.42\ \%$$

This indicates again the capacity of the experiment to detect a variation in the response factor average of ± 4.42 %, which is greater than the precision previously imposed. This increment is essentially due to preliminary underestimation of the error standard deviation. Using the obtained S_e, to reach the initial precision, in the same condition, we should perform at least N=2n=128 trials (Φ=0.04/0.0608=0.658). This number would be reducible only at the cost of some power of the test.

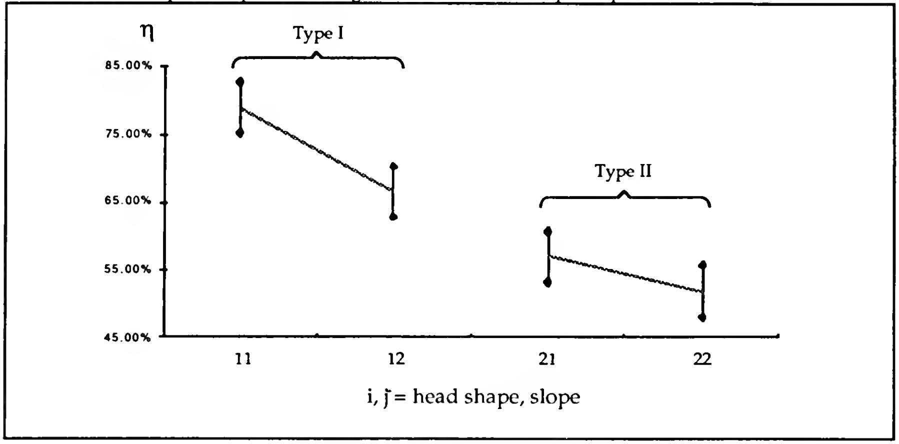
Multiple Comparisons

Another useful technique to test the effects of factors and interactions included in the framework is the multiple comparisons test. This has the advantage to be easy to calculate and consents to introduce the Experimentalwise (E) error, which is the probability of wrongly rejecting at least one null hypotheses when making statistical tests of two or more null hypotheses using the data from a single experiment [Mason R.L., 1989]. The relationship between the experimentalwise error and the comparisonwise (α) error defined previously is:

$$E = 1 - (1 - \alpha)^{k-1}$$

where k is the number of population means compared. From this expression, it is clear that, even with a low comparisonwise error (i.e. α=0.05), we can reach a high experimentalwise error if the number of comparisons increases. The comparison among the four averages of the head shape-slope interaction is reported in the PFJ example (Exhibit 12). The Bonferroni procedure was used to select the comparisonwise error ($\alpha/2k$), which is resulted to be approximately 0.005 [Ibid.].

Exhibit 12. Multiple comparison diagram for the head shape-slope interaction .



The Least-Significant-Interval plotted using the Bonferroni procedure is $LSI=\{t_{[\alpha/2k,\ v]}\ (2/n)^{1/2}\ S_e\}/2= 0.0377$. From the diagram it is possible to observe the effect of interaction as difference of the slopes of the two segment linking the averages. Also there is an evident partial overlapping of intervals, which indicate the difficulties to distinguish clearly the effects. This is due to more strict criterion adopted by this method (Appendix E).

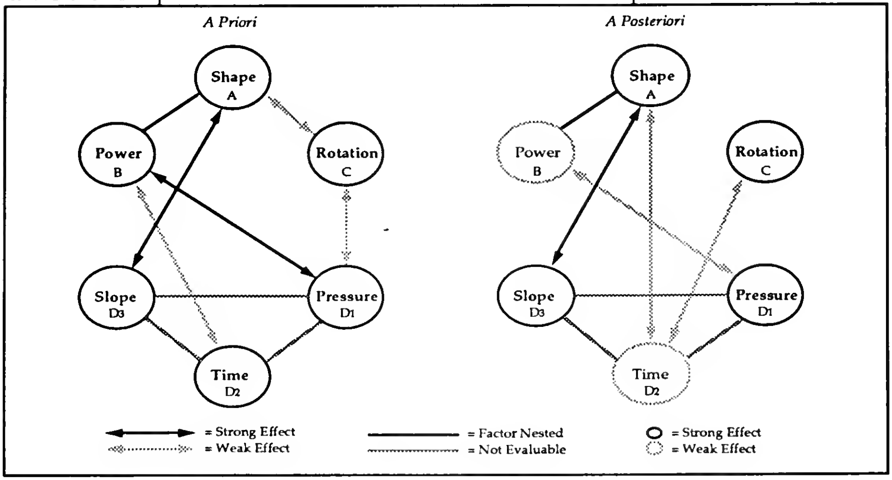
5 Closing the Optimization Design Loop

The most impressive result was the impact of head-design on the final performance. The head was responsible for more than 40% of the total variation. In addition, the better head design (type II) reduces the interaction effect with slope factor, making the product even more robust for the user (Exhibit 12). While the technology factor, the head rotation, - more complex to embody in the product- affected only 2% of the observed variation.

The experiment created new knowledge that went to increase the previous level. To illustrate this improvement it is useful to compare the interactions table before and after the experiment (Exhibit 13). In this exhibit we distinguish immediately the update knowledge and, more important, we can understand better the phenomenon by trying to explain the difference between our results and our expectations.

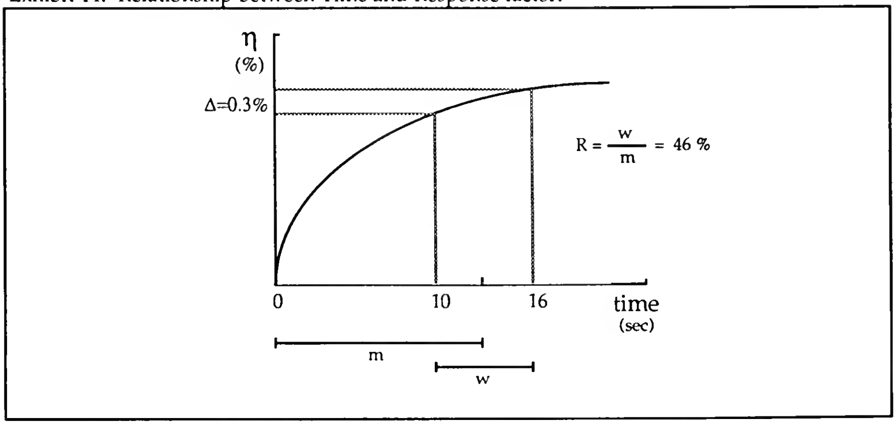
The PFJ experiment revealed that the time factor was not significant, and since it is intuitive that between a very short time (1 sec) and a longer time (10 sec) there must be a difference, we have a row deduction of the curve relationship between response and the time.

Exhibit 13. Comparison of interactions tables before and after the experiment.



From this curve (Exhibit 14) we observe that in reality the range chosen (R=46%) was not big enough to detect a change, but from a practical point of view this implies that excluding short uses of the product the performance is quite robust with respect to time. Analogous inferences can be made for the power factor.

Exhibit 14. Relationship between Time and Response factor.



The final, but no less important, information created is a better estimation of the error standard deviation. This allows more precise planning of any subsequent experiments.

6 Conclusion

Although the concept of quality keeps evolving [Gehani R.R., 1993] and many quantitative techniques have been developed for optimizing products, little emphasis has been placed on identifying all their possible applications.

This work is a further attempt to move *quality by design* upstream, by introducing in the experimental phases factors typically neglected and otherwise not easily evaluable. Nevertheless, in the improving-through-learning process, it is necessary to place the proper emphasis on statistical competencies, which have to complement engineering skills.

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Appendix A

Sums of Squares Calculation

The formulas used in the calculation of the sums of squares are reported to show the different approach necessary with factor B nested in factor, A and the mixed model of Full Factorial for Factors A, B, C and Half Fractional for factors D1, D2, D3, which here implies that the interactions D_iD_j (for every ij) are not evaluable.

Defining the generic response observation as Y_{ijklmn}, with indexes corresponding to factors A, B, C, D1, D2, D3 and varying between 1 and 2, we start the first step with calculation of the Correction Factor (CF) [Montgomery D.C., 1984]:

$$CF = Y^2_{......}/2^5$$

where dots indicate that indexes have been totalized. Then the Total sum of squares can be expressed as:

$$SS_{tot} = \Sigma_i \Sigma_j \Sigma_k \Sigma_l \Sigma_m \Sigma_n \; Y^2_{ijklmn} - CF$$

and all factors and interactions sums of squares are:

$$SS_A = \Sigma_i Y^2_{i.....}/2^4 - CF$$

$$SS_{B(A)} = \Sigma_i \Sigma_j Y^2_{ij....}/2^3 - \Sigma_i Y^2_{i.....}/2^4$$

$$SS_C = \Sigma_k Y^2_{..k...}/2^4 - CF$$

$$SS_{AC} = \Sigma_i \Sigma_k Y^2_{i.k...}/2^3 - CF - SS_A - SS_C$$

$$SS_{CB(A)} = \Sigma_i \Sigma_j \Sigma_k Y^2_{ijk...}/2^2 - \Sigma_i \Sigma_j Y^2_{ij....}/2^3 - \Sigma_i \Sigma_k Y^2_{i.k...}/2^3 + \Sigma_i Y^2_{i.....}/2^4$$

$$SS_{D1} = \Sigma_l Y^2_{...l..}/2^4 - CF$$

$$SS_{D2} = \Sigma_m Y^2_{....m.}/2^4 - CF$$

$$SS_{D3} = \Sigma_n Y^2_{.....n}/2^4 - CF$$

$$SS_{AD1} = \Sigma_i \Sigma_l Y^2_{i..l..}/2^3 - CF - SS_A - SS_{D1}$$
$$SS_{AD2} = \Sigma_i \Sigma_m Y^2_{i...m.}/2^3 - CF - SS_A - SS_{D2}$$
$$SS_{AD3} = \Sigma_i \Sigma_n Y^2_{i....n}/2^3 - CF - SS_A - SS_{D3}$$

$$SS_{B(A)D1} = \Sigma_i \Sigma_j \Sigma_l Y^2_{ij.l..}/2^2 - \Sigma_i \Sigma_j Y^2_{ij....}/2^3 - \Sigma_i \Sigma_l Y^2_{i..l..}/2^3 + \Sigma_i Y^2_{i.....}/2^4$$
$$SS_{B(A)D2} = \Sigma_i \Sigma_j \Sigma_m Y^2_{ij..m.}/2^2 - \Sigma_i \Sigma_j Y^2_{ij....}/2^3 - \Sigma_i \Sigma_m Y^2_{i...m.}/2^3 + \Sigma_i Y^2_{i.....}/2^4$$
$$SS_{B(A)D3} = \Sigma_i \Sigma_j \Sigma_n Y^2_{ij...n}/2^2 - \Sigma_i \Sigma_j Y^2_{ij....}/2^3 - \Sigma_i \Sigma_n Y^2_{i....n}/2^3 + \Sigma_i Y^2_{i.....}/2^4$$

$$SS_{CD1} = \Sigma_k \Sigma_l Y^2_{..kl..}/2^3 - CF - SS_C - SS_{D1}$$
$$SS_{CD2} = \Sigma_k \Sigma_m Y^2_{..k.m.}/2^3 - CF - SS_C - SS_{D2}$$
$$SS_{CD3} = \Sigma_k \Sigma_n Y^2_{..k..n}/2^3 - CF - SS_C - SS_{D3}$$

The sum of squares for the residual can be calculated as difference between the total sum of squares and the sum of all sums of squares previously calculated:

$$\begin{aligned} SS_{Residual} = {} & SS_{tot} - SS_A - SS_{B(A)} - SS_C - SS_{D1} - SS_{D2} - SS_{D3} - SS_{AC} - SS_{AB(A)} \\ & - SS_{AD1} - SS_{AD2} - SS_{AD3} - SS_{B(A)D1} - SS_{B(A)D2} - SS_{B(A)D3} - \\ & - SS_{CD1} - SS_{CD2} - SS_{CD3} \end{aligned}$$

It could be easily demonstrated, developing the formulas above shown, that the effect of the nested factor B is confounded with the interaction between the factor A and factor B [Montgomery D.C., 1984]:

$$SS_{B(A)} = SS_B + SS_{AB}$$

and analogous considerations can be made for any interaction with the nested factor B, for example:

$$SS_{B(A)C} = SS_{BC} + SS_{ABC}$$

and so on.

The value of the correction factor was CF= 12.934 and in Table A1 are reported the values assumed by the variables appearing in the previous sums, in function of their levels.

Table A1. Sums of Experimental Observations

	[1]	[2]
Yi.....	11.649	8.695
Y.j....	nested	nested
Y..k...	9.795	10.549
Y...l..	11.351	8.993
Y....m.	10.194	10.150
Y.....n	10.864	9.480

	[11]	[12]	[21]	[22]
Yij....*	5.939	5.710	4.456	4.239
Y..kl..	5.582	4.213	5.769	4.780
Y..k.m.	4.807	4.988	5.387	5.162
Y..k..n	5.225	4.570	5.639	4.910
Yi.k...	5.723	5.926	4.072	4.623
Yi..l..	6.355	5.294	4.996	3.700
Yi...m.	5.731	5.918	4.463	4.232
Yi....n	6.316	5.332	4.548	4.147

	[111]	[112]	[121]	[122]
Yijk...	2.942	2.996	2.781	2.929
Yij.l..	3.138	2.800	3.217	2.493
Yij..m.	2.928	3.011	2.803	2.906
Yij...n	3.295	2.644	3.021	2.689
	[211]	[212]	[221]	[222]
Yijk...	2.177	2.280	1.895	2.344
Yij.l..	2.528	1.928	2.467	1.772
Yij..m.	2.301	2.155	2.162	2.077
Yij...n	2.351	2.105	2.197	2.043

The numbers on the top rows indicate the values assumed by the indexes; for example, [211] means i=2, j=1 and k=1.

Appendix B

Percent Contribution Calculation

The variance calculated in the fixed levels experiment for a factor or interaction, listed in Anova Table, contains some amount of variation due to the error [Phadke M.S, 1983; Ross P.J., 1988]. The generic factor variance observed (V_x) can be written as:

$$V_x = V^*_x + V_e$$

where V^*_x is the variance due solely to the factor x and V_e is the variance of the error. The pure variation of factor x can be isolated as:

$$V^*_x = V_x - V_e$$

and since in the Anova Table the variance is expressed as the ratio of sum of squares and degree of freedom of factor ($V_x=SS_x/\nu_x$) we have:

$$SS^*x/\nu_x = SS_x/\nu_x - V_e$$

Solving for SS^*_x we obtain:

$$SS^*_x = SSx - V_e \times \nu_x$$

then the percent contribution of the factor x (Π_x) respect the total variation expressed in terms of sum of squares can be calculated as:

$$\Pi_x = SS^*_x / SS_{Tot} \times 100$$

The contribution of error (residual) then is calculated replacing on it all variation removed by the factors and interactions:

$$\Pi_e = (SS_x + dof_{F.I.} \times V_e) / SS_{Tot} \times 100$$

where dof $_{F.I.}$ is the total number of degree of freedom available for factors and interactions.

This correction is necessary to avoid overestimating the contribution of the effects, and it is cause of the presence of negative number in the initial Anova Table (14a).

Appendix C

Regression Analysis

The regression analysis, conducted with the MINITAB® software , confirms the results obtained with the Anova Analysis for the factors and for the model, then it produces residual and fitted values necessary for the Residual Analysis, the regression results are listed in Table C1:

Table C1. Regression Table

Predictor	Coef.	St..Dev.	t-ratio	p-Value	VIF
Constant	0.840	0.026	31.760	0.000	
A	-0.221	0.031	-7.240	0.000	2
C	0.047	0.022	2.180	0.038	1
D1	-0.147	0.022	-6.830	0.000	1
D3	-0.123	0.031	-4.020	0.000	2
A*D3	0.073	0.043	1.690	0.103	3

Source	dof	SS	MS	F	p-Value
Regression	5	0.535	0.1069	28.67	0.000
Error	26	0.097	0.0037		
Total	31	0.631			

The regression equation using the coefficients shown in Table C1 is:

$$\eta = 0.840 - 0.221\,A + 0.047\,C - 0.147\,D_1 - 0.123\,D_3 + 0.073\,AD_3$$

The typical indicators of the goodness of the model, as the R^2 and the R^2_{adj}, are below reported :

$$R^2 = 84.6\ \% \qquad \text{and} \qquad R^2_{adj} = 81.7\ \%$$

The software also provides other information as: the absence of evidence of Lack Of Fit ($p > 0.1$) and the pure error test:

$$F = MS_{LOF}/MS_{pe} = 1.27 \quad \{\text{p-Value} = 0.325\}$$

where the pure error (pe) has 16 degree of freedom.

The residuals obtained from the regression analysis are listed in Table C2, where it is also shown fitted values, sorted residuals and normal quantile values.

Table C2. Residuals of the final models.

Trials	Residuals	Fits	Sort. Res.	n-quantil
1	-0.038	0.468	-0.098	-2.070
2	-0.036	0.569	-0.089	-1.643
3	0.067	0.616	-0.077	-1.396
4	0.041	0.692	-0.072	-1.213
5	0.098	0.840	-0.070	-1.064
6	0.012	0.569	-0.061	-0.935
7	0.002	0.569	-0.047	-0.820
8	-0.038	0.692	-0.038	-0.716
9	0.043	0.616	-0.038	-0.618
10	0.047	0.421	-0.036	-0.527
11	0.003	0.717	-0.034	-0.440
12	-0.025	0.421	-0.028	-0.356
13	-0.077	0.471	-0.025	-0.274
14	0.033	0.518	-0.016	-0.195
15	-0.014	0.764	-0.014	-0.116
16	-0.061	0.666	0.002	-0.039
17	0.002	0.617	0.002	0.039
18	-0.089	0.739	0.003	0.116
19	0.070	0.739	0.012	0.195
20	0.101	0.518	0.013	0.274
21	0.032	0.717	0.024	0.356
22	-0.098	0.764	0.032	0.440
23	-0.034	0.840	0.033	0.527
24	-0.070	0.468	0.041	0.618
25	-0.047	0.619	0.043	0.716
26	-0.016	0.569	0.047	0.820
27	0.090	0.617	0.066	0.935
28	-0.072	0.887	0.067	1.064
29	0.013	0.666	0.070	1.213
30	0.024	0.887	0.090	1.396
31	0.066	0.619	0.098	1.643
32	-0.028	0.471	0.101	2.070

Appendix D

Residuals Analysis

Using the regression results, it is possible to perform the residuals analysis with the intent to test the basic assumptions of the Anova Analysis and to investigate if some unpredicted sources of variation are detected.

The assumptions to test are: independence, normality distribution and homogeneity of errors. The first, and most important, can be tested with the Durbin-Watson method, which requires the calculation of the statistic:

$$d = \Sigma_i (r_i - r_{i-1})^2 \ / \ \Sigma_i r^2_i \qquad \text{with } i = 2, 3,\ldots,32$$

where r_i is the residual, and the value $d^*=1.80$ obtained must be compared with the bounds d_u and d_L (1.18, 1.73 for $\alpha=0.05$) from the tables available in literature [Mason R.L., et al., 1989]. To test the hypothesis H_o: errors not autocorrelated vs H_a: errors positively autocorrelated, we can: I) reject H_o if $d< d_L$, II) do not reject H_o if $d>d_u$; III) draw no conclusion if $d_L<d<d_u$. Analogous reasoning is required for negative autocorrelation. In the FPS case $d^*>d_u$ and we did not reject H_o, which means that the independence assumption was supported.

The normality assumption can be tested using the Shapiro-Wilk method, which requires to order ascending the residuals and calculate the statistic:

$$W = b^2 \ / \ S^2$$

where $b = \Sigma_i a_i r^2_i$ and $S^2 = \Sigma_i (r_i - \Sigma_i r_i/n)^2$ with $i=1,2,\ldots,n$ and from the tables [ibid.] we obtain the values a_i and the critical value for W. The hypothesis of normality is rejected if the value W obtained is less then W-critical. The value for the FPS case was $W^*=0.966$, that compared with the $W_{(n=32,\ \alpha=0.45)}= 0.965$ did not allow us to reject the hypothesis of normality.

Independence and homogeneity assumptions can be further investigated plotting the residuals vs the factors and interactions (exhibit D1), vs fitted values and time (exhibit D2, D3); the normality assumption with the plot of the residuals vs the fitted values and the normal quantile values (exhibit D3, D4). These do not show any pattern to worry about. On the other hand, they do not help in finding other missed sources of variation.

Table D1. Residuals vs Main Factors.

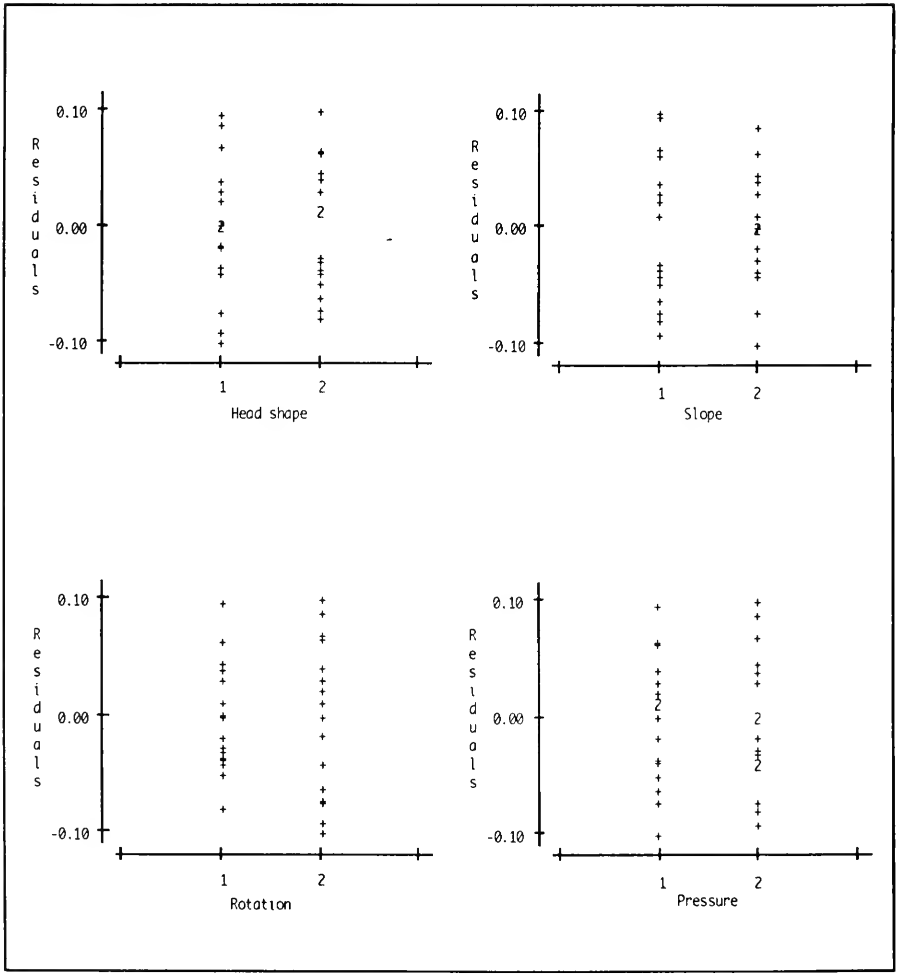


Exhibit D2. Plot Residuals vs Experimental Time.

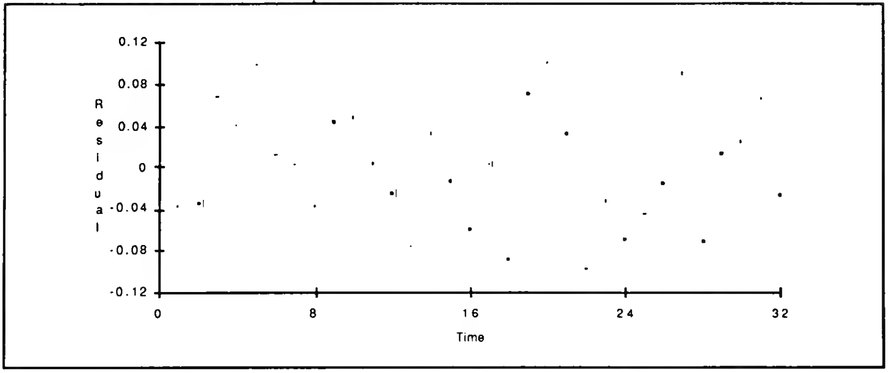


Exhibit D3. Plot Residuals vs Fitted Values.

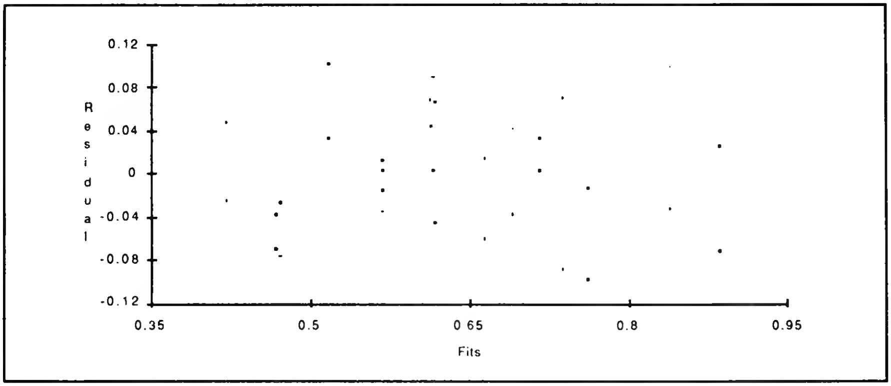
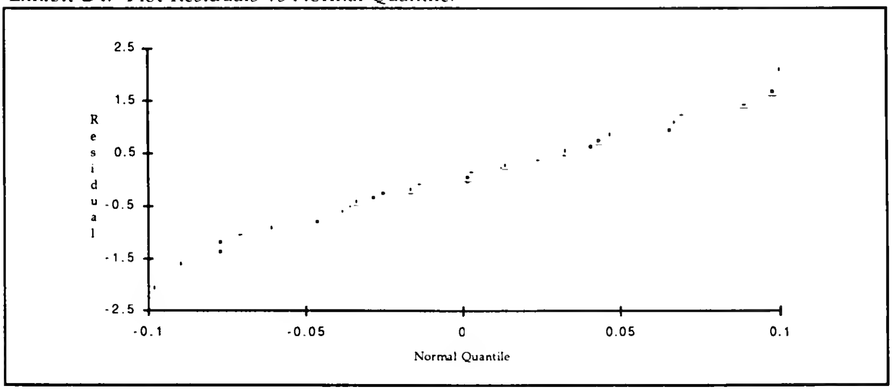
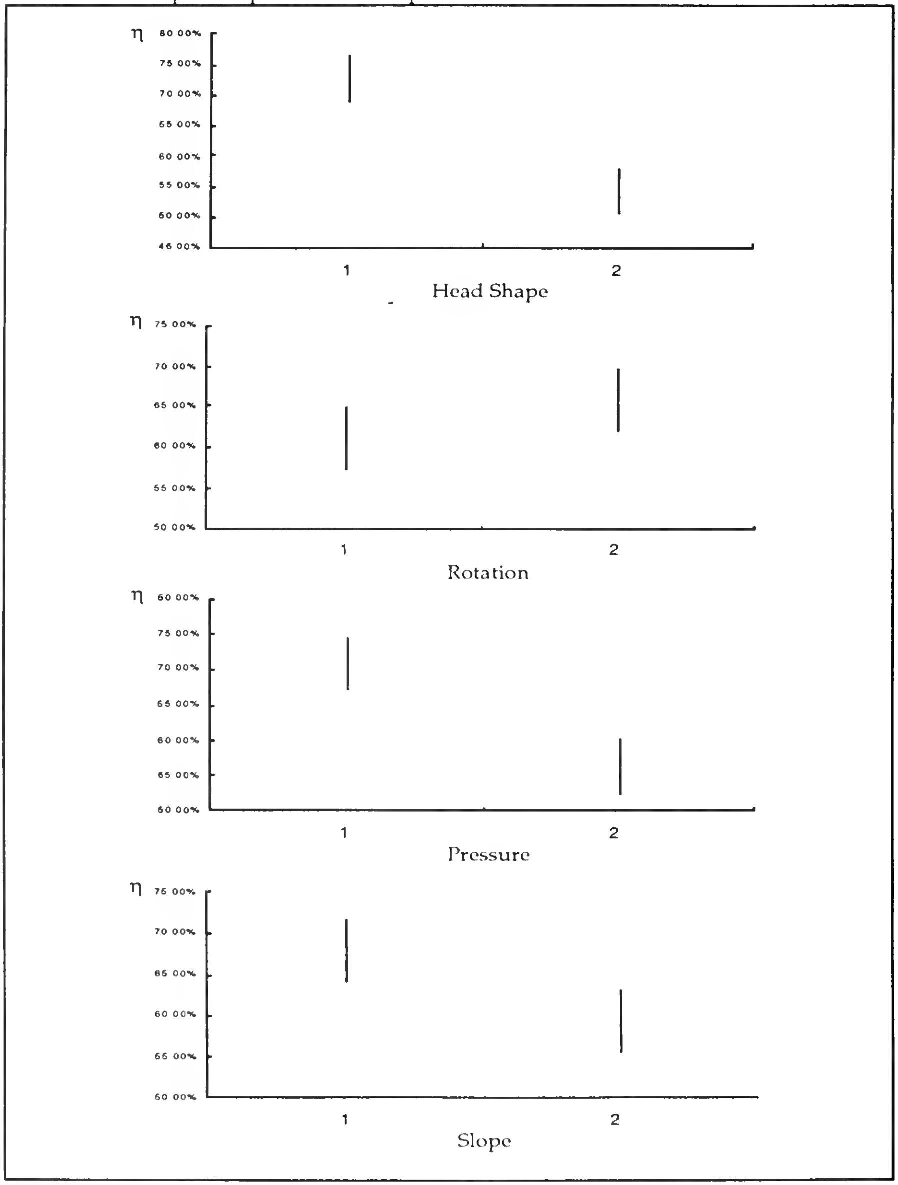


Exhibit D4. Plot Residuals vs Normal-Quantile.



Appendix E

Exhibit E1. Multiple ComparisonsPlots: response factor vs main factors.



Appendix F

Table F1. Sample size requirements for tests on difference of two means from independent normal population, equal population standard deviations.

	Level of t-Test																
single-side		α=0.005				α=0.01				α=0.025				α=0.05			
double-side		α=0.01				α=0.02				α=0.05				α=0.1			
β =		0.01	0.05	0.1	0.2	0.01	0.05	0.1	0.2	0.01	0.05	0.1	0.2	0.01	0.05	0.1	0.2
	0.30																
	0.35																102
	0.40												100			108	78
	0.45				118				101			105	79		108	86	62
	0.50				96			106	82		106	86	64		88	70	51
	0.55			101	79		106	88	68		87	71	53	112	73	58	42
	0.60		101	85	67		90	74	58	104	74	60	45	89	61	49	36
	0.65		87	73	57	104	77	64	49	88	63	51	39	76	52	42	30
	0.70	100	75	63	50	90	66	55	43	76	55	44	34	66	45	36	26
	0.75	88	66	55	44	79	58	48	38	67	48	39	29	57	40	32	23
	0.80	77	58	49	39	70	51	43	33	59	42	34	26	50	35	28	21
	0.85	69	51	43	35	62	46	38	30	52	37	31	23	45	31	25	18
	0.90	62	46	39	31	55	41	34	27	47	34	27	21	40	28	22	16
	0.95	55	42	35	28	50	37	31	24	42	30	25	19	36	25	20	15
	1.00	50	38	32	26	45	33	28	22	38	27	23	17	33	23	18	14
	1.10	42	32	27	22	38	28	23	19	32	23	19	14	27	19	15	12
	1.20	36	27	23	18	32	24	20	16	27	20	16	12	23	16	13	10
$\phi=\lvert\delta\rvert/\sigma$	1.30	31	23	20	16	28	21	17	14	23	17	14	11	20	14	11	9
	1.40	27	20	17	14	24	18	15	12	20	15	12	10	17	12	10	8
	1.50	24	18	15	13	21	16	14	11	18	13	11	9	15	11	9	7
	1.60	21	16	14	11	19	14	12	10	16	12	10	8	14	10	8	6
	1.70	19	15	13	10	17	13	11	9	14	11	9	7	12	9	7	6
	1.80	17	13	11	10	15	12	10	8	13	10	8	6	11	8	7	5
	1.90	16	12	11	9	14	11	9	8	12	9	7	6	10	7	6	5
	2.00	14	11	10	8	13	10	9	7	11	8	7	6	9	7	6	4
	2.10	13	10	9	8	12	9	8	7	10	8	6	5	8	6	5	4
	2.20	12	10	8	7	11	9	7	6	9	7	6	5	8	6	5	4
	2.30	11	9	8	7	10	8	7	6	9	7	6	5	7	5	5	4
	2.40	11	9	8	6	10	8	7	6	8	6	5	4	7	5	4	4
	2.50	10	8	7	6	9	7	6	5	8	6	5	4	6	5	4	3
	3.00	8	6	6	5	7	6	5	4	6	5	4	4	5	4	3	
	3.50	6	5	5	4	6	5	4	4	5	4	4	3	4	3		
	4.00	6	5	4	4	5	4	4	3	4	4	3		4			

Note: The entries in this table show the number of observations needed (for each of two samples of equal size) in a test of significance of the difference between two means in order to control the probabilities of the errors of the first and second kinds at a and b respectively.

Date Due

APR 2 9 1998		
MAY 3 1 1998		